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Joint cognition and the role of human agency in random number choices

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Abstract

Joint cognition refers to the mental systems that support group performance when carrying out a shared, or jointly owned task. We focused here on understanding the social configurations that underpin key phenomena in joint cognition; in particular, whether individual cognition in task-sharing environments is mostly shaped by social factors or not. To this end, we investigated, first and mainly, whether human presence is necessary for the creation of joint performance; second and separately, whether prior experience of task sharing has an adaptive influence on subsequent individual choices; and third and additionally, whether individual differences in a social trait mediate joint performance. We describe an experiment in which participants combined with another human or a computer as they attempted to generate a paired sequence that was as random as possible. First, we found little difference in joint performance with regard to whether a human or a computer was the co-participant, except for immediate repetitive response. Second, we found evidence for choice adaptation, but only under the lower time pressure. Third, we replicated previous research in which no systematic link was established between social desirability and joint performance. We conclude that joint cognition phenomena may be rooted primarily in turn-taking configurations rather than in social dynamics per se.

Introduction

Allport, Styles, and Hsieh (1994) noted that a standard, almost monolithic, approach to the study of cognitive phenomena at the time was to require individuals to carry out a single task, repeatedly, for a long period of time. This was designed to facilitate the accumulation of a large and stable pattern of data that could both address the mechanics of cognition and constrain theoretical models of performance. Allport et al. (1994) pointed out the severe limitation – even artificiality – of this paradigmatic approach, since real-world behavior does not involve such singular, repetitive activity. Nor is it the case that changes in cognitive processes, when they happen, result from an external signal. As a solution, Allport et al. advocated (using a paradigm championed also by Rogers & Monsell, 1995 and reviving work pioneered by Jersild, 1927) the analysis of task-switching, requiring at least two sets of stimulus-response mappings to be ready for deployment, being juggled so that one might be implemented at any time. Task switching has turned out to be a highly complex, but also immensely rich approach to the study of executive control (e.g., Kiesel et al., 2010). Yet there is an alternative solution to the methodological straight-jacket characterized by Allport et al. (1994) – which has received much less analysis. Alongside task-switching, one might consider *task-sharing* as a complementary approach to the repetition of a single task completed by a single person. That is, rather than deploying two tasks to be completed by a single person, one can envisage one task completed by more than one person. In such a configuration, multiple individuals contribute to a common objective. (i.e., instead of a mapping of *1 individual : several task sets*, one has a mapping of *1 task set : several individuals*). An overarching question then becomes; how do different individuals contribute to task performance? That is, how does task-sharing shape individual cognition?

A related field is that of joint action (see Sebanz, Bekkering, & Knoblich, 2006), which focuses on how individuals coordinate their actions – that is motoric behaviors. Joint action research is an important and vibrant contemporary field (Vesper et al., 2017). However, often in joint action, each performer has a different role, which at some level interacts with their partner's role. To provide a real-world example, team sport players may have a common objective but take different responsibilities and coordinate them in order to achieve the common purpose.

More relevantly, recent research has begun to address how executive functions operate in social settings, through analysis of a joint task-switching paradigm (e.g., Dudarev & Hassin, 2016; Liefooghe, 2016; Wenke et al., 2011; Yamaguchi, Wall, & Hommel, 2017a, 2017b;). Dudarev and Hassin (2016) as well as Liefooghe (2016) demonstrate switch costs occur even when one participant shifts to and from the irrelevant task assigned to the other participant. According to the authors, this is taken to suggest that people not only represent the task sets of others but also track the task processes in their minds. Consequently it is argued that executive functions are used in participant / task switching. In these studies, however, task switching is yoked and therefore confounded with participant switching. In order to overcome this, Yamaguchi et al. (2017b) asked two participants to engage in the same two tasks and estimated task switch costs both when the participants switched from one trial to the next and when one participant repeated consecutive trials by implementing a within-participants design. The authors described evidence for task switch costs only when the same participant repeated trials but not when participants switched. This provides strong evidence against the claim that people represent task sets of relevant others even if they do not need to coordinate those tasks in joint task settings (e.g., Knoblich, Butterfill, & Sebanz, 2011). Yamaguchi et al. (2017a) also reported that task switch costs are present even when participants switched in a goal-sharing condition where two co-participants acknowledged that they were sharing the effects / results of their actions. Such a finding confirms the importance of shared task goal in joint task settings. Whilst this recent research on joint task switching offers a valuable insight with respect to social engagement of executive functions, in these joint task-switching studies each participant is not asked to optimize group performance by intentionally and adaptively integrating their choices with those from someone else to form a group, or emergent product. To draw again on the analogy of a team sport, joint task switching does not require any reaction to what other team members do.

In contrast, Towse, Towse, Saito, Maehara, and Miyake (2016) explored the sharing of a cognitive task among multiple people, each contributor playing an equal and equivalent part in the net performance. They asked participant dyads to generate a combined random number sequence, a known executive function paradigm (see Cooper, 2016; Towse, 1998). Random sequence production can be completed individually, but the task structure makes it highly amenable to being shared by a dyad, especially when compared with group activities like idea generation (Nijstad & Stroebe, 2006). Towse et al. found strong evidence for a phenomenon of “response contagion”. That is, when participant “A” produced a particular response choice, participant “B” in following on with the next choice would be likely to produce an associated value. The combined sequence contained more occurrences of neighboring values (e.g., 1 followed by 2, 8 followed by 7) than sequences produced by the dyad members tested alone at the same overall pace. Since pairs alternated responses, this shows the extent to which each person incorporated or represented their partner’s response. Their partner’s response set up even stronger prepotent continuation choices than their own immediately preceding response did. In addition, the combined sequence also contained few immediate response repetitions, which is the case also for individually produced sequences. That is, almost invariably participants do not repeat their own response choices in random sequences (see Towse, 1998; Towse & Neil, 1998) and when operating as a dyad, individuals strongly avoided repeating what their partner said too.

In studying how cognition is shaped by a collaborative environment, Towse et al. (2016) also explored whether the pattern of performance was dependent on their being an interacting pair. In Experiment 2, they examined performance when a participant combined with an Experimenter to produce combined sequences. The participant was fully aware that they were turn-taking with the Experimenter. It was also visibly apparent that the Experimenter choices came from reading a list of responses on paper. Therefore, the Experimenter was clearly *not* being influenced by the participant. Nonetheless, participants made sequence choices in this *semi-interactional* configuration that closely aligned those of the *fully-interactional* configuration of Experiment 1.

Among several questions not fully addressed by Towse et al. (2016), a key issue is the extent to which human agency within the dyad is paramount. Therefore, our first objective for the present study is to determine if a human partner is a necessary and sufficient condition for the creation of an interactional context that leads to response contagion aforementioned. Participants were co-present with another person – albeit known as an Experimenter and visibly reading from a typed sheet in Experiment 2 of Towse et al. (2016). Perhaps the human presence over-rode any top-down situational knowledge and effectively created an interactional environment for the participant. We suggest this is a contemporary issue for joint action research also (Dolk et al., 2014a), where the notion of co-representation of stimulus-response mappings has been analyzed by deploying partners hidden behind a screen (Obhi & Hall, 2011), humanoid robots (Stenzel et al., 2012) or with a Japanese waving cat (Dolk, Hommel, Prinz, & Liepelt, 2014b). We address this issue by asking participants to generate combined random number sequences with in one condition a computer (a non-interactional, non-human being) and in another condition a confederate (a human, purportedly interactional partner). We aim to establish the extent to which human interaction is required for collaborative effects. Notably, we implement a within-participant design to expose participants to both interactional configurations, and facilitate their comparison.

Relating to the first objective, we predicted that in the present study, using the same paradigm as Towse et al. (2016), joint sequences – specifically with respect to stereotyped choice pairs – would be less random when the partner is another human than when a non-human computer. In other words, the sequence quality in joint performance would be impaired compared with baseline and individual performance when the partner is a human than when a computer. We predicted that features of joint performances including response contagion observed in Towse et al. (2016) were amplified by human presence. Since we are not aware of any data or theory directly examining agency effects in joint random generation, here we reason by analogy to joint action research. Many studies have investigated agency effects with joint Simon paradigms and indicated that intentionality and perceived interpersonal similarity attributed to others (e.g., Stenzel et al., 2012; Müller et al., 2011) elicit a joint Simon effect, in the form of slower reaction times on incompatible stimulus-response trials than compatible stimulus-response trials. This allows us to infer that in a joint Simon task, interacting with another human that is believed an intentional agent imposes larger executive functions demands than when interacting with a computer that is believed an unintentional agent. If this generalizes to the present context of joint random generation settings, paired performance with another human partner will be less random than with a non-human computer. This in turn would support a more social view of joint cognition whereby human presence as a partner automatically recruits executive functions (Dudarev & Hassin, 2016).

A second objective focuses on specifying the nature of response adaptation and learning that potentially results from experience in joint performance. The second objective examines longer-term impacts of task-sharing on individual cognition between experimental conditions, while the first objective for partner's agency effect examines on-going impacts of task-sharing on individual cognition within one experimental condition. More specifically, we investigated whether *choice adaptation* is observed in joint cognition. Choice adaptation refers here to the uninstructed change in choice decisions, in other words self-directed learning from one's recent experience. Evidence for choice adaptation would be important for computational accounts of random generation task performance – by indicating the plasticity in response selection - and likewise for accounts of executive function performance constraints (the role of more direct or *explicit* instruction in random choices is interesting but is separate – see Brugger (1997) for an early review, and Neuringer (1986) for an exemplar dataset). Choice adaptation is theoretically important for understanding random generation behavior, because it demonstrates that random generation is sufficiently malleable that response decisions are not only affected by participant's pre-existing response associations that are functionally permanent on the one hand, as well as the immediately preceding choices that is functionally transient on the other hand. It would suggest that in addition, we should recognize short to medium term experiences during the experiment.

Choice adaptation is also important theoretically for understanding joint cognition, because it provides an additional empirical phenomenon of the sensitivity to the choices a partner makes. To our knowledge, however, choice adaptation has received relatively little attention in the literature on random generation this far. We suggest this is because experimental contexts have not been designed to pick up on medium term changes in choice behavior. When a random sequence is collaboratively generated by two genuine participants, of course one has very limited control over each contributor's choices, and thus the opportunity for systematically studying choice adaptation is limited. And we know from studies that individuals are at least partially predictable (i.e., not random) when producing sequences – so in the dyad, each likely experiences similar biases from another as they exhibit themselves.

Towse et al. (2016) discussed some highly tentative evidence for choice adaptation. As already noted, responses are rarely repeated in human sequences. However, in Experiment 2, participants experienced the Experimenter repeating their choices, because the Experimenter used a quasi-random sequence that was truly independent of the participant. Participants who gave individual sequences *after* the paired sequences – and therefore after experiencing other-repeats – tended to self-repeat more than those participants who gave individual sequences first, and so had yet to experience the dyadic environment. Yet this order effect, whilst moderate in size (η^2 =.107) was not statistically reliable. Therefore, in the current study we can investigate the possibility of choice adaptation more systematically in the confederate condition, since certain digram permutations can be expected to occur and participants thus encounter such possibilities, apparently when they are working within a collaborative setting. We ask whether participants can use this experience to modify their selections when they perform individually. Considering statistical non-significance of a task order effect in Towse et al. (2016), we suspect that choice adaptation is in any case a subtle effect. It might be necessary to inspect whether choice adaptation occurs more closely – for each response pace. Because slower response pace generally admits of more deliberate adaptation of responses in random generation by individual (Baddeley, 1966; Jahanshahi, Dirnberger, Fuller, & Frith, 2000; Towse, 1998), participants in a slow pace task might be likely to apply repetitive responses learned in dyadic environments to their own responses.

A third objective for the present study focused on individual differences. Towse et al. (2016) also collected ancillary data from participants in the form of a Social Desirability Scale (SDS; Crowne & Marlowe, 1960), in order to investigate whether conjoint performance in general, and social interactional alignment in particular, varied as an individual-difference trait, and then did not find any reliable correlations. Towse et al. (2016) however examined individual differences between conjoint random generation performance and self-reported social desirability only for American and British participants (Experiment 1) but not for Japanese participants (Experiment 3). Japanese generally put greater importance on group harmony, group goals, and working in groups compared to Americans, although Japanese are not more collectivistic than Americans as a whole (Oyserman, Coon, & Kemmelmeier, 2002). Is there a possibility that individual difference in social desirability mediates conjoint performances among Japanese, who putatively think much of group performance? The current study recruited Japanese participants. Given the orientation towards human agency in the current study, we administered the social desirability measure as a potentially relevant individual difference construct. We addressed whether there is evidence for a pattern of individual differences on the quality of random generation that links to personality traits.

In summary, through experimental investigation concentrating on three objectives, we aimed at better understanding how task-sharing shapes individual cognition as well as extending the research of Towse et al. (2016); primarily, we wish to make clear whether individual cognition in task-sharing environments is affected mostly by social factors or not. The first and foremost objective is to examine the contribution of social interaction and human agency in joint cognition; specifically, to investigate whether characteristics of joint performance in a random generation paradigm would be ascribed to acting with a human partner (i.e., considerable difference in performances between with a human partner and with a computer partner) or solely to turn-taking irrelevant to the presence of a human partner (i.e., little difference in performances between with a human partner and with a computer partner). The second objective is to address the notion of choice adaptation in performance; specifically, to investigate whether experience of immediate repetitions that the partner produced in joint environments would bring about an increase in self repetition for the subsequent individual environments or not. The third objective is to investigate whether joint performance with a human partner would vary as individual difference in social desirability or not. For simplicity and comparability with motivating work, participants were also asked to generate random number sequences in this study.

Method

Participants

Sample size was estimated on the basis on Towse et al. (2016), in which 40, 32, and 34 participants took part in Experiments 1 - 3, respectively. Therefore, we decided to recruit 40 participants in this experiment, taking a few data exclusions into consideration. Forty students from Kyoto University took part in this experiment (mean age = 21.0 years; 27 females and 13 males) and received ¥500 worth of book coupon. Before participation, all participants provided a written consent form, which stated their right to withdraw the experiment whenever they would like for any reason.

There was no local ethics committee or formal ethical approval process instituted at the point of data collection. However, this study was conducted in accordance with the

Declaration of Helsinki and the Ethical Principles of the Japanese Psychological Association (JPA) and the American Psychological Association (APA). Additionally, the experimental protocol closely followed those described in Towse et al. (2016) with respect to ethical issues, and Experiments 1 and 2 of the previous study had been approved by two institutional ethics committees.

Procedure

All participants initially completed the Social Desirability Scale (SDS; Crowne & Marlowe, 1960), which is a popular instrument consisting of 33 yes-no question items to measure whether individuals attempt to behave according to desirable social norms in various daily situations. We evaluated whether individual orientation towards social alignment would be linked to joint cognition performance with a human partner.

Afterwards, they produced four random number generation sequences. Following Towse et al. (2016), two individual sequences were generated at a slow (*individual slow* condition – one response every 3 seconds) or fast pace (*individual fast* condition – one response every 1.5 seconds). A third sequence was generated jointly with another “participant” who was actually a confederate (*joint-confederate* condition), and the fourth sequence comprised joint production with a computer (*joint-computer* condition). In the two joint production conditions, *combined* response rate was the same as the individual-fast condition and thus, because of alternation, *individual’s* response rate was the same as for the individual-slow condition. The two individual sequences were blocked and counterbalanced within the block. The order of tasks (blocked individual, joint-confederate, joint-computer) was counterbalanced.

In all conditions, following Towse et al. (2016), participants were asked to produce as random a sequence as possible using numbers between 1 and 10. Participants were encouraged to imagine repeatedly rolling a 10-sided dice and reporting the numbers that the dice displayed (the use of dice imagery is common in random generation instructions). Participants were instructed to select the 10 alternatives equally often and avoid fixed sequence patterns. An auditory tone (a beep signal) emitted by the computer set a regular interval during which participants should verbally announce a number. The importance of maintaining response pace was emphasized. All of the participants' number choices were written down on data sheets by an Experimenter.

In both *individual* conditions, participants performed the tasks alone and generated 100 number responses between 1 and 10 in as random an order as possible.

In the *joint-confederate* condition, each participant received explanations about the task prior to the entry of another participant, actually a confederate, into the room. Participants were instructed to generate a sequence by giving a number alternately with their partner and to make the combined sequence as random as possible. Participants were also told that they must not speak to their partner so as to minimize bias caused by individual impressions and that their partner was also instructed in the same way by another experimenter next room. These instructions enabled us to control participants' social interaction; actually, all participants nodded but did not exchange a single word to the confederate. Participants sat down across a desk from their partner and took it in turns again to generate a number between 1 and 10 to accompany a beep tone every 1.5 seconds. A monitor was set up to be over the left shoulder of the participant and another monitor over the right shoulder of the confederate (see Fig. 1a). Each person could see the monitor behind their partner but not the monitor behind them. For each participant response, an asterisk was presented on the monitor behind the partner synchronously with a beep tone and then participants verbally produced a number on these signals. For each response turn of the confederate, a number was presented on the monitor behind the participant with a beep tone and the confederate simply read it aloud. Participants ought to perceive the partner watching the monitor as natural, because they would expect in their partner's turn an asterisk cue to be shown on their monitor, as was the case for them. The confederate number sequence was pre-prepared and constant for all dyads (generated using the RANDBETWEEN function of Microsoft Excel 2007). After a practice event in which participants and the confederate together contributed 20 numbers (10 numbers each alternately), participants tackled the main task in which they made a combined sequence consisted of 200 numbers (100 numbers each alternately).

In the *joint-computer* condition, participants were instructed to generate a sequence by giving a number alternately with a PC and make the combined sequence as random as possible. Participants sat down in front of a desktop PC and a monitor (Fig. 1b) and took it in turns to generate a number between 1 and 10, synchronously with a beep tone, the participant generating a response every 3 seconds to accompany what the computer produced. For each response turn of the participant, an asterisk appeared on the monitor with a beep tone and then participants produced a number on this signal. For each computer turn, the computer produced a number using as female voice accompanying a beep tone; at that time, there was no image presented on the monitor. Although participants were told that the computer randomly selected a number every time, the computer reproduced the same pre-prepared sequence deployed in the joint-confederate condition. Therefore, the same pre-prepared sequence was used for all participants both in the joint confederate and joint computer task. After a practice trial in which participants and the PC made a combined sequence consisted of 20 numbers (10 numbers each alternately), participants tackled the main trial in which they made a combined sequence consisted of 200 numbers (100 numbers each alternately).

After completing all four random number generation sequences, participants were asked whether they had been aware that the partner just read out a number that was displayed on the monitor behind them. Two participants (one female and one male) mentioned some suspicion that this was the case. Their data were therefore excluded from analysis, yielding 38 participants in the full dataset. Finally, participants were debriefed.

Measures

Human random number sequences provide us with many potential clues about the executive functioning profile of those who produce such responses. Whilst many randomness indices can be calculated, they can be categorized into a few clusters that reflect different types of executive functioning (Miyake et al., 2000; Towse & Neil, 1998). Following Towse et al. (2016), we implement an analysis using four performance indices—*Digram Use*, *Adjacency*, *Immediate Repetition*, *Redundancy*—that are representative of each type of executive functioning, and available through RGCalc (Towse & Neil, 1998, which documents full computational descriptions).

Digram Use (or RNG; Evans, 1978) is a measure of stereotyped sequencing, which identifies repeated occurrences of two-item permutations among all responses. Digram Use values increases as paired combinations are preferentially emitted, which means a higher value indicates less randomness. *Adjacency* represents the frequency of two neighboring numbers such as 2, 3 and 8, 7. Adjacency score increases as such combinations appear, which means the higher value indicates the less randomness. Digram Use and Adjacency are argued to be potentially sensitive to different types of prepotent response inhibition: the former reflects inhibition of individual, more idiosyncratic combinations while the latter reflects inhibition of conventional responses for all individuals. *Immediate Repetition* represents the frequency of the same numbers selected twice in succession such as 5, 5. When participants perform random generation without specific restriction, Immediate Repetition scores are invariably much lower than they ought to be in an ideal random sequence (theoretically, Immediate Repetition appears about 10% of all sets of two consecutive responses in a truly random sequence). *Redundancy* expresses the evenness of the response frequency distribution. While the above three indices certainly reflect prompt responses to the preceding choice, Redundancy reflects maintenance and monitoring of a relatively long-term sequence of choices. Redundancy score increases as specific alternatives are generated more often than others, which means the higher values indicates the less evenness.

Results

We specify all data exclusions, all manipulations, and all measures in the study (Simmons, Nelson, & Simonsohn, 2012). The raw data from this study can be accessed from the following URL: http://XXX [URL blinded for review]. For transparency, the raw data also identifies and includes the two participants noted above who showed some suspicion of the confederate's status (i.e., full sample size $N = 40$). The raw data comprise the random sequences from each participant.

Influence of a human partner in joint random generation

The first objective of this experiment is to examine *social* influences of a human partner on executive functioning in a joint task. Accordingly, we directly compare performance in the *joint-confederate* and *joint-computer* conditions. However, we also follow the analytic approach from Towse et al. (2016) whereby true pair sequences (from actual participant dyads) are compared with *composite* sequences. Such composite sequences were obtained for each participant by artificially merging the individual slow sequence and the pre-prepared confederate / computer sequence. These composite productions form a baseline condition insofar as they represent joint choices from two truly independent sources. That is, they included the same pre-prepared sequence as for the joint tasks, but participant choices produced individually.

The left side of Table 1 summarizes means and standard deviations of randomness indices derived from RGCalc using all 200 responses in joint sequences. However, joint sequences include the pre-prepared response set of confederate / computer, so that indices calculated from all responses might be biased by these non-participant items. Therefore, in order to estimate participants' performance in a more impartial way, we also calculated each index on the basis solely of participants' responses. To calculate Digram Use, Adjacency, and Immediate Repetition based on participants' decisions, we used only response pairs each of which consists of a response of participant following a pre-prepared response of confederate / computer, so as to identify specifically the extent to which *participants* gave a stereotypical response, a numerical neighbors, and the identical number to their partner's response, respectively. That is, given a joint sequence CPCPCPCP..., where C is a pre-prepared response of confederate / computer and P is a genuine participant response, we calculated these three indices from only CP bigrams and not PC bigrams. With regard to Redundancy, we used only the responses of *participants* by excluding the pre-prepared sequence from the joint and composite sequences. That is, given the above sequence CPCPCPCP..., we calculated this new Redundancy index from a sequence including only P (i.e., PPPPPP...). The right side of Table 1 summarizes means and standard deviations of randomness indices calculated from only participants' responses in joint sequences. For distinction, labels "- All" and "- Only" were attached to joint randomness indices calculated from all 200 responses and only participants' responses, respectively.

A series of one-way within-participants ANOVAs (confederate vs. computer vs. composite) was then performed for each performance index as dependent variable, and omnibus effects were established in each case.

Digram Use - All scores differed across conditions, F (2, 74) = 5.01, p = .009, η^2 = .119; composite sequences were significantly more random (exhibiting lower scores) than both the joint-confederate [t (37) = 2.52, p = .016, η^2 = .144] and the joint-computer condition [t (37) = 3.30, p = .002, η^2 = .230], while the latter two conditions did not differ significantly [t (37) = 0.49, p = .631, η^2 = .006]. We then analyzed Digram Use - Only scores and found almost identical pattern of results to the above, F (2, 74) = 16.69, p < .001, η^2 = .311; the composite sequences were significantly more random (exhibiting lower scores) than both the joint-confederate [t (37) = 4.09, p < .001, η^2 = .314] and joint-computer sequence [t (37) = 5.29, p < .001, η^2 = .436], while the latter two conditions did not reach significance [t (37) = 1.70, p = .097, η^2 = .073].

Adjacency - All scores also differed across conditions, F (2, 74) = 3.24, p = .045,

η^2 = .080; joint-confederate sequences were significantly more random (exhibiting lower scores) than joint-computer sequences [t (37) = 2.91, p = .006, η^2 = .185] but joint-confederate and joint-computer sequences were not significantly different from the composite sequences [t (37) = 0.36, p = .718, η^2 = .004 and t (37) = 1.68, p = .102, η^2 = .073]. We then analyzed Adjacency - Only scores and did not find a significant difference across conditions, F (2, 74) = 0.34, p = .717, η^2 = .009. Thus, the Adjacency advantage of the joint-confederate condition over the joint-computer condition among all responses was not uniquely observed in participants' decisions.

Immediate Repetition - All scores also differed across conditions, F (2, 74) = 44.19, p < .001, η^2 = .544; the frequency of immediate repetition in the composite sequences was significantly and substantially greater than both in the joint-confederate and joint-computer sequences [t (37) = 7.29, p < .001, η^2 = .593 and t (37) = 8.27, p < .001, η^2 = .656]. Also, the joint-confederate sequences had significantly more repetitions than the joint-computer sequences [t (37) = 2.35, p = .024, η^2 = .130]. To examine this further, we then analyzed Immediate Repetition - Only scores. Participant's repetitions in joint task settings are so rare that the data distribution is highly skewed and thus we employed parametric and non-parametric tests. A one-way within-participants ANOVA showed a significant difference across conditions, F (2, 74) = 168.68, p < .001, η^2 = .818. The frequency in the composite sequence was significantly greater than both in the joint-confederate and joint-computer conditions [t (37) = 13.40, p < .001, η^2 = .830 and t (37) =

13.48, $p < .001$, $\eta^2 = .832$]; furthermore, the frequency of immediate repetition in the confederate condition was marginally significant than in the computer condition [t (37) = 1.96, $p = .058$, $\eta^2 = .096$]. A non-parametric Friedman test for the same dataset also showed a significant difference across conditions, χ^2 (2) = 58.53, $p < .001$. Wilcoxon's signed rank tests showed much more repetitions in the composite sequence than both in the joint-confederate and joint-computer conditions [$T = 0.0$, $z = -5.23$, $p < .001$ and $T = 2.0$, $z = -5.34$, $p < .001$] and also a significant increase in the frequency of immediate repetition in the joint-confederate condition compared to in the joint-computer condition [$T = 90.0$, $z = -1.98$, $p = .047$]. Thus, the Immediate Repetition advantage of the joint-confederate condition over the joint-computer condition among all responses remained in participants' decisions.

Redundancy - All scores also differed across conditions, F (2, 74) = 5.60, $p = .005$, $\eta^2 = .131$; composite sequences were significantly and substantially more random (exhibiting lower scores) than both joint-confederate and joint-computer sequences [t (37) = 3.29, $p = .002$, $\eta^2 = .230$ and t (37) = 2.81, $p = .008$, $\eta^2 = .176$] but joint sequences were not significantly different from each other [t (37) = 0.05, $p = .958$, $\eta^2 < .001$]. Further, we conducted analyses on Redundancy - Only scores and found the same pattern as the above, F (2, 74) = 12.21, $p < .001$, $\eta^2 = .248$; composite sequences were significantly and substantially more random (exhibiting lower scores) than both joint-confederate and joint-computer sequences [t (37) = 4.11, $p < .001$, $\eta^2 = .314$ and t (37) = 4.47, $p < .001$, η^2

= .348] but joint sequences were not significantly different from each other [t (37) = 0.39, p = .700, η^2 = .004].

In summary, (1) joint sequences were generally less random than the composite baseline irrespective of the partner's agency, (2) joint performance with respect to prepotent response inhibition (i.e., Digram Use and Adjacency) and sequence monitoring (i.e., Redundancy) did not differ as a function of whether the partner was a human or a computer, and (3) the only difference in performance between a human partner and a computer partner appeared in Immediate Repetition.

Comparisons between individual and joint sequences

Next, this section focuses the characteristics of joint performance in comparison with individual performance, in contrast with the preceding section focusing on comparison with ideal baseline performance (i.e., composite sequences). To do so, we compared performance in the joint and individual sequences. This also established whether the current results are different or not from Experiment 1 of Towse et al. (2016), in which participants in a dyad were both naive and interacting cooperatively. We use as a randomness measure in the joint condition the mean of the two randomness scores calculated from the first 100 and second 100 responses in each joint sequence, because it is inappropriate to compare directly randomness indices from different response sequence lengths. Table 2 summarizes mean randomness scores based on 100 responses of the individual and joint sequences. A series of one-way within-participants ANOVAs (individual-slow vs. individual-fast vs.

joint-confederate vs. joint-computer) were conducted for each index. Detailed statistics of subsidiary analyses (post-hoc *t*-tests or Wilcoxon's signed rank tests) were summerized in Appendix Table A1.

Digram Use scores differed across conditions, $F\ (3, 111) = 23.49$, $p < .001$, $\eta^2 = .388$. Subsidiary analyses revealed no significant differences between joint-confederate and joint-sequences and a marginal difference between individual-slow and individual-fast sequences; but all the other comparisons were significant. Therefore the order of randomness regarding Digram Use is as follows: individual-fast ≤ individual-slow < joint conditions. This pattern of results is consistent with Towse et al. (2016).

Adjacency scores also differed across conditions, $F\ (3, 111) = 21.98$, $p < .001$, $\eta^2 = .373$. Subsidiary analyses revealed that all pairwise comparisons were significant, except a marginal difference between the individual-fast and joint-computer conditions. Therefore the order of randomness regarding Adjacency is as follows: individual-fast ≤ joint-computer < joint-confederate < individual-slow. This pattern of results is also consistent with Towse et al. (2016).

Immediate Repetition scores also differed across conditions, $F\ (3, 111) = 179.91$, $p < .001$, $\eta^2 = .829$. Subsidiary analyses revealed that all pairwise comparisons were significant except individual-slow vs. individual-fast. Therefore the frequency of Immediate Repetition is as follows: individual conditions < joint-computer < joint-confederate. This pattern of results is *not* consistent with Towse et al. (2016), who indicated that individual and joint (mutually cooperative) conditions did not differ in Immediate Repetition. A non-parametric Friedman test for the same dataset confirmed a significant difference across conditions, χ^2 (3) = 98.21, p < .001. Subsidiary non-parametric comparisons produced an identical pattern of significant outcomes to the above parametric comparisons.

Redundancy scores also differed across conditions, F (3, 111) = 3.47, p = .019, η^2 = .086. Subsidiary analyses revealed that the numbers generated in the individual-slow condition were more evenly distributed than in the other three conditions, which did not differ from each other. Therefore, the order of randomness regarding Redundancy is as follows: joint conditions = individual-fast < individual-slow. This pattern of results is *not* consistent with Towse et al. (2016), who indicated that the number distribution was more even in the joint condition than in the individual conditions.

In summary, we replicated two key phenomena regarding individual random generation performance: (1) digit sequences generated at fast response pace were generally more predictable than those generated at a slow response pace (e.g., Baddeley, 1966), but (2) Immediate Repetition was not sensitive to response speed (e.g., Towse, 1998). These replications validate the individual tasks in this study. Moreover, we replicated the two findings regarding the relationship between the individual and joint performances (Towse et al., 2016): (1) Adjacency scores both in joint-confederate and joint-computer sequences were worse than those in individual-slow sequences and (2) Adjacency and Digram Use scores both in joint-confederate and joint-computer sequences were better than those in individual-fast sequences. These results suggest that both costs and benefits from joint performance involving a human partner in Towse et al. (2016) were replicated with a computer partner. However, we also found two different points from previous findings regarding the relationship between the individual and joint performances: (1) number distributions represented by Redundancy scores were more even in individual sequences than in joint sequences and (2) Immediate Repetition in joint sequences was more frequent than in individual sequences.

Participants' individual contribution to joint random generation

We also examine how participants regulate their own responses within joint sequences. That is, rather than a focus on the full sequence, here we investigate individuals' contribution to a joint production environment. To do so, we showed "repetition lag," which is the response distance between each alternative and its subsequent reappearance. In an example sequence: 7, 10, 4, 2, 2, 5, 7, the repetition lag of 7 is 6 and the repetition lag of 2 is 1 (= Immediate Repetition). By giving a graphic representation of repetition lags, the pattern of sequence history and turn-taking configuration are visually clarified.

Fig. 2a shows the lags between repetitions of response alternatives in joint sequences. The zig-zag patterns are clear for short lags in all sequences, where the low frequency of even repetition lag indicate that participants remembered they had said themselves and avoided saying that number soon. However, the zig-zag waves of confederate and computer sequences are generally smaller and disappear at earlier lag values compared to composite sequences. In fact, we conducted a series of one-way within-participants ANOVAs for the frequency of repetition lag at each point of repetition lag 3, 4, 5, 6, and 7 in order to examine whether amplitudes of the zig-zag waves are statistically different across conditions. The frequency of repetition lag significantly differed across conditions at lag three, F (2, 74) = 12.03, $p < .001$, $\eta^2 = .245$, four, F (2, 74) = 11.57, $p < .001$, $\eta^2 = .238$, five, F (2, 74) = 3.89, $p = .025$, $\eta^2 = .095$, and six, F (2, 74) = 49.57, $p < .001$, $\eta^2 = .573$, but not at lag seven, F (2, 74) = 1.59, $p = .211$, $\eta^2 = .041$. At all data points of lag from 3 through 6, subsidiary analyses (post-hoc t-tests; see Appendix Table A2 for detailed statistics) showed that there was no significant difference between the confederate and computer conditions, but the composite condition significantly (or marginally) from both the confederate and computer conditions. These results indicate that peaks and troughs in the zig-zag pattern within confederate and computer sequences are reliably smaller than those of composite sequences. Further, in order to examine whether the zig-zag waves converge sooner for confederate and computer sequences than for composite sequences, we conducted a two-way within-participants ANOVA for the frequency of repetition lag with sequence conditions (confederate vs. computer vs. composite) and repetition lags (7 vs. 8 vs. 9) as factors. There was a significant interaction between the two factors, F (4, 148) = 13.88, $p < .001$, $\eta^2 = .273$. For the confederate condition, the repetition frequency differed across lags, F (2, 74) = 3.52, $p = .035$, $\eta^2 = .087$; however, the lag 7 and 8 frequencies did not significantly differ from each other [t

(37) = 0.15, p = .880, η^2 < .001]. For the computer condition, the repetition frequency did not differ across lags, F (2, 74) = 1.07, p = .349, η^2 = .028. In summary, the repetition lag contours of confederate and computer sequences between lag 7 and 9 do not form a v-shaped valley. For the composite condition, on the other hand, the repetition frequency differed across lags, F (2, 74) = 47.52, p < .001, η^2 = .562; furthermore, the frequencies of lag 7, 8, and 9 significantly differed from each other [lag 7 vs. 8, t (37) = 11.02, p < .001, η^2 = .774 ; lag 7 vs. 8, t (37) = 5.00, p < .001, η^2 = .410 ; lag 8 vs. 9, t (37) = 4.22, p < .001, η^2 = .325]. In summary, the repetition lag contours of composite sequences between lag 7 and 9 forms a distinct v-shaped valley. Therefore, the zig-zag waves of confederate and computer sequences disappear earlier than that of composite sequences. These different patterns in repetition lag indicate that participants regulated or adapted their own responses when contributing to a combined sequence. There are usually few short-lag repeats in true joint sequences produced by two collaborative participants, while many short-lag repeats appear in composite sequences (Towse et al., 2016). In the current experiment, of course, confederates and computers produced responses oblivious to the participants' choice so that short-lag repeats increased for the confederate and computer sequences.

Fig. 2b shows the lags between repetitions of self responses in joint sequences (only even lags in Fig. 2a). The profile of confederate and computer conditions are almost identical; in fact, there was no significant difference between confederate and computer conditions for all data points from lag 2 through 20 in Fig. 2b, ts (37) < 1.17, ps > .250, η^2

$< .036$ with small effect sizes. This indicates that participants maintained and referred to their past responses equivalently in the two joint conditions – the presence of human agency in the joint settings did not matter here. The confederate and computer graphs in Fig. 2b are similar to the graphs in Fig. 2c, which illustrate repetition lags of individual tasks. All of those graphs show relatively small number of short-lag repeats and a frequency peak around eight. This suggests that participants in joint conditions, so as to optimize the net performance, attempted to expand interval of repetition in the same way as they did in individual conditions.

Next, we derived participant-in-pair sequences by extracting only participants' responses from joint sequences. In other words, we removed the confederate and computer contributions from the joint sequences. We then calculated the four performance indices and the repetition lags already described. All four randomness measures are summarized in Table 3. They did not show significant differences between partner type, *t*s (37) < 1.60, *p*s > .120, $\eta^2 < .063$ with small to medium effect sizes. The repetition lags were illustrated in Fig. 3. The repetition profile for both joint conditions are very similar, in particular for short-lag repeats (from 1 through 4); in fact, there was no significant difference between the two joint conditions at all data points from lag 1 through 20 in Fig. 3, *t*s (37) < 1.68, *p*s > .101, $\eta^2 < .073$ with small to medium effect sizes, excepting for lag 6, t (37) = 1.88, $p = .068$, $\eta^2 = .090$, and lag 14, t (37) = 2.03, $p = .050$, $\eta^2 = .102$. These results suggest that participants did not generally change their own response style in joint conditions regardless of whether their partner was a human or a computer.

Choice adaptation: Influence of task order on Immediate Repetition

The second objective of this study was to examine whether participants adapted their choice behavior as an experiential consequence of joint task environments. We hypothesized that a high incidence of Immediate Repetition in the joint conditions might have the effect of increasing Immediate Repetition for subsequent individual productions, in particular at a slow response pace, through experiential feedback or choice adaptation. Accordingly, we compared Immediate Repetition in the individual conditions as a function of whether this occurred before or after engaging in joint conditions[1]. For slow responses, there were significantly more Immediate Repetitions after completing joint conditions (M = 1.09, SD = 1.72) than before (M = 0.20, SD = 0.54), t (28) = 2.25, p = .032, η^2 = .152[2]. For fast responses, the increase in Immediate Repetition after completing joint conditions (M = 1.17, SD = 2.22) compared to before (M = 0.40, SD = 1.02) was not significant, t (33) = 1.42, p = .166, η^2 = .058[3]. Both effects were in the same direction, but these data imply some degree of strategic application of repetition choices, which was therefore less

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[1] A two-way mixed ANOVA for individual Immediate Repetition with task order (before and after joint conditions; between-participants) and response pace (slow and fast; within-participants) as factors indicated that an interaction between the two factors was not significant, F (1, 36) = 0.07, p = .787, η^2 = .002. We suggest this is because variance of Immediate Repetition is commonly too large. However, we suggested the necessity of closer inspection of choice adaptation and then set up a specific hypothesis that slow response pace, but not fast response pace, in individual task helped participants intentionally apply repetitive responses that they had experienced in joint task. Therefore, we administered separate analyses for the slow and fast response pace.

[2] Mann-Whitney's U test: U = 124.0, z = 1.81, p = .070.

[3] Mann-Whitney's U test: U = 143.5, z = 1.09, p = .278.

prevalent / systematic in the fast condition under greater time pressure.

However, such a task-order effect might simply occur as a time effect; that is, Immediate Repetition might increase in joint sequences after experiencing individual tasks[4]. We then compared Immediate Repetition - Only scores in the joint conditions as a function of whether this occurred before or after engaging in individual conditions. For joint-confederate sequences, Immediate Repetition after completing individual conditions (M = 2.67, SD = 2.96) was not significantly different from before (M = 1.59, SD = 1.80), t (36) = 1.43, p = .161, η^2 = .053[5]. For joint-computer sequences, Immediate Repetition after completing individual conditions (M = 1.60, SD = 2.95) was not significantly different from before (M = 1.89, SD = 2.65), t (36) = 0.32, p = .754, η^2 = .003[6]. Thus, prior experience of individual tasks did not influence repetition choices in subsequent joint tasks.

With respect to Immediate Repetition, we examined more generally whether there were order effects within the two joint conditions. For the frequency with which participants repeated a human partner's response, there was a non-significant difference between those who experienced the joint-computer condition in advance (M = 2.65, SD = 2.48) and those who did not (M = 1.78, SD = 2.49), t (36) = 1.05, p = .299, η^2 = .029[7]. For the frequency with which participants repeated a computer's response, there is also non-

[4] We thank a reviewer for identifying this possibility.
[5] Mann-Whitney's U test: U = 142.5, z = 1.08, p = .280.
[6] Mann-Whitney's U test: U = 158.5, z = 0.68, p = .495.
[7] Mann-Whitney's U test: U = 138.5, z = 1.24, p = .215.

significant difference between those who experienced the joint-human condition in advance (M = 1.56, SD = 2.95) and those who did not (M = 1.90, SD = 2.53), t (36) = 0.38, p = .708, η^2 = .004[8]. Thus, choice adaptation identified from joint to individual conditions was not observed systematically within the two joint production environments.

Influence of social desirability on joint performance

The third objective of this study was to examine whether individual differences in social traits modulated joint performances. To do so, we examined correlation coefficients between the total score of SDS and the different randomness indices derived from the joint-confederate and joint-computer sequences in this study. There was only a marginal correlation coefficient to the joint-confederate Digram Use - Only score, which was calculated from only response pairs each of which consists of a response of participant subsequent to a pre-prepared response of confederate, r (36) = .314, p = .055, but otherwise we did not find any significant correlation coefficients, rs (36) < .236, ps > .153. The lack of systematic associations replicates the outcomes reported in Towse et al. (2016).

Discussion

This study used a joint shared task that is known to tap executive functions and aimed at better understanding how task sharing shapes individual cognition by extending the findings of the previous study (Towse et al., 2016). In particular, we intended to clarify

[8] Mann-Whitney's U test: U = 159.5, z = 0.65, p = .516.

whether individual cognition in task-sharing environments is affected mostly by social factors or not. To do so, we investigated whether joint performance differed when participants partnered another human and when they partnered a computer. When considered from the perspective of the overall, combined task performance, contrary to our initial prediction, results imply that participants behaved in comparable ways regardless of agency type, that is whether their partner was a human or computer. That is, both costs and benefits from joint performances in comparison with the baseline and individual performances were observed in the computer partner condition as well as in the human partner condition. This supports a less social view of joint cognition where various phenomena unique to joint cognition may be rooted primarily in turn-taking configurations rather than social dynamics per se. This in turn suggests that effects identified in Towse et al. (2016), in both semi-interactional and fully-interactional configurations, were not simply social effects arising from the co-presence of another individual, that is being dependent on the presence of another human agency. Nonetheless, this broad conclusion needs to be nuanced by important, more specific findings that have also been identified, as well as noting some caveats. We found that the frequency of immediate repetition increased when alongside a human partner compared to a computer partner. This suggests that the presence of a human partner enabled or stimulated participants, at least in part, to regulate their responses differently compared to their coupling alongside a computer partner. This also tells us that Immediate Repetition can be a sensitive measure that tracks the influence of partner's agency, namely social factors other than cognitive demands of the task itself.

Though the literature on joint action argues that social factors such as perceived interpersonal similarity affects how participants perform the task, studies are inconsistent with respect to whether a human co-actor and a computer co-actor have different consequences for behavior (Stenzel et al., 2012). This might be because joint action can sometimes be measured just by reaction time. Other performance metrics, that capture behavioral choices, might show influences from social dynamics that do not feature in chronometry. Joint random generation paradigm provides multiple measures that reflect different aspects of executive control; indeed the current study offers clues as to which aspects of joint performance are affected by social factors and which are not. Nevertheless, those various metrics used here were not mediated by individual differences in social desirability. The current data collected from Japanese participants, consistent with the conclusions from data collected from Western participants in Towse et al. (2016), provide no systematic evidence to suggest that sociability – at least as measured by the specific index of the Social Desirability Scale – is an individual difference metric that affects cognitive choices in a minimally social situation even among the Japanese who putatively belong to a collectivistic culture where they attach importance to group harmony. However, it might be valuable for future research to consider the relevance of other social traits such as empathy (Davis, 1983), social anxiety (Mattick & Clarke, 1998), and autistic spectrum traits (Baron-Cohen, Wheelwright, Skinner, Martin, & Clubley, 2001) in order to characterize more fully the social factors that affect joint cognition performance.

Analyzing how different measures are sensitive to different social configurations leads us to ask; why was Immediate Repetition of random sequences influenced by the presence (or absence) of a human partner? We speculate that one of the contributory factors is that human presence promoted social learning and imitation. Social learning refers to changes in rules for responding to stimuli that are derived from the observation of another individual (Hoppitt & Laland, 2013) and can be more strongly triggered while people are observing human action compared to non-human robot action (Press, 2011). In the current study, participants witnessed the human partner repeating their response more frequently than they had normally expected, which encouraged them to understand that they did not have to avoid repetitions, so they became more likely to imitate their partner and produce repetitions during the joint-confederate condition. While the human partner's repetitions might have seemed intentional, participants might have inferred that the computer's repetitions were accidental. Hence, the computer's repetitions did not become a cue to increase the participant's repetitions during the joint-computer condition.

Towse et al. (2016) concluded that when participants share performance with another person on an executive functioning task, and co-produce a response sequence, performance is different from that elicited under individual circumstances. At the same time, a key conclusion from their analyses is that participants also experience sequence contagion. That is, participants treat their partner's responses as if it were their own, in at least two respects. First, they are very reluctant to repeat their partner's choice, much in the same way that participants show strong repetition avoidance of their own selections. Second, they also are inclined to make selections that have numerical associations with their partner's choice. That is, they struggled to resist prepotent sequence combinations even where these combinations are co-constructed within the dyad.

With respect to the first line of evidence, we find that the repetition lag profile in the joint-confederate condition – where we assume the participant believed that they were collaborating with another person – was very similar to the profile in the joint-computer condition – where it was evident to participants that they were in a non-interactional environment. Furthermore, both conditions differed from a composite analysis where there was no co-production of sequences in real time. This suggests that joint cognition leads to modification of response behavior, but human agency is not (always) a necessary condition for these changes to take place.

Otherwise, we must assume participants somehow infer agency to computer sequences. We note that computer sequences were generated by the computer using a human female voice. This was a deliberate design choice for matching with the joint-confederate condition. It seems unlikely, but perhaps this led participants to ascribe some human agency to the computer as a result. The natural human voice might be enough for participants to feel social dynamics even in absence of physical presence (in the real world, this is true for an audio telephone call), even though interacting with a human more strongly activates social cognitive network of the brain than interacting with a lap-top computer and humanoid robot (Krach et al., 2008).

With respect to the second line of evidence, we examined the extent to which response pairs are preferentially selected. Digram Use did not differ between the joint-confederate and joint-computer sequences, but joint conditions did differ from composite sequences. As above, task-sharing affected response choice, but human agency was not particularly critical to the trajectory it took. In contrast, from the perspective of the more specific Adjacency values, that is neighboring numeric values only, joint-confederate sequences showed fewer associated pairs than joint-computer sequences. However, the difference in the Adjacency values was not observed when analyzing only participant choices. Again, bearing mind the caveats above, we suggest that human agency may not be a necessary condition for the behavioral changes.

A separate objective of this study was to investigate choice adaptation – how choices are affected by prior experience of what others say and do. This objective focused on longer-term influence of task-sharing to examine how prior experience of shared-task environments affects individual performance in the subsequent experimental conditions, while the initial objective for partner's agency focused on shorter-term influence of task-sharing to examine how the presence of a partner affects individual performance within one condition. Towse et al. (2016) noted the modest sample size for their analysis of choice adaptation and found some tentative, but not statistically significant, evidence that participants modify their production behavior based on their shared interactions under joint cognition. In the present study, we found that after being exposed to repetitions in a joint production environment, there was a trend for increased repetition performance individually, which is more straightforward evidence that choice adaptation can take place. But in addition, we also see evidence that such adaptation may be a conscious strategic, volitional and thus time-dependent adaptation. This leads us to speculate that such adaptation may be rather transient – if there was a gap for example of something like 24 hours between joint and individual productions, the adaptation might be at the very least dissipated. This would be consistent with evidence from individual random sequence production (Towse & Mclachlan, 1999: Experiment 3), albeit from children, where instructions that emphasize the relevance and legitimacy of response repetitions can lead to a "bump" in lag-1 repetitions, whilst other repetition distances are less affected. A strategic choice to "insert" more repetitions does not change the full profile of repetition behavior.

In conclusion, this study showed that, despite a few exceptional phenomena, joint cognition arising from a shared or jointly owned task is less social than we usually expect. Many characteristics of individual cognition in joint cognitive tasks might not be shaped by effects of social factors such as presence of an interactive partner. Furthermore, we also indicated that task-sharing impacts individual cognition over different time scales. At the shortest time scale, within an experimental condition, a partner in front of participants has a substantial influence in that they commonly make predictable choices (i.e., response contagion: more frequent neighboring values and common digram pairings). At a longer time scale, between experimental conditions, there is adaptation or change such that different signals can become more common (i.e., choice adaptation: the increased number of immediate repetitions in individual sequences). Finally, we believe that the current study emphasizes and reinforces the potential for task-sharing to go beyond the hitherto common paradigm of studying cognition amongst individuals only, and restricting questions to those in which individuals represent the atomic level of analysis. This does not detract from the potential for studies of the individual to yield important insights about cognition, whether an individual repeats the same task time and time again, or whether and individual switches between different task sets (Allport et al., 1994). Nonetheless, our analysis confirms the additional insights and opportunities that follow from a systematic analysis of joint cognition and the way in which collaborative, coordinated, or multi-person activities can shape cognition.

Compliance with ethical standards

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Table 1. Mean randomness scores based on all 200 responses in joint sequences [left three columns] and those based on only responses of participants in the joint sequences [right three columns] (SDs in parentheses)

	Based on all 200 responses of the joint sequences [All]			Based on only the responses of participants [Only]		
	Joint-Confederate	Joint-Computer	Composite	Joint-Confederate	Joint-Computer	Composite
Digram Use	0.321 (0.016)	0.323 (0.014)	0.314 (0.011)	0.254 (0.030)	0.265 (0.038)	0.227 (0.022)
Adjacency	17.70 (2.39)	19.04 (3.29)	17.89 (2.69)	18.50 (5.63)	18.34 (7.00)	17.66 (3.71)
Immediate Repetition	14.82 (4.09)	12.71 (4.30)	21.03 (4.23)	2.24 (2.55)	1.74 (2.78)	10.95 (3.18)
Redundancy	0.461 (0.277)	0.458 (0.367)	0.282 (0.173)	1.645 (1.446)	1.746 (1.614)	0.604 (0.476)

The composite Redundancy score based on only the responses of participants is identical to that of the individual-slow condition, because the composite sequences consist of the pre-prepared confederate / computer's sequence and participant's individual-slow sequences.

Table 2. Mean randomness scores based on 100 responses of individual sequences and those based on 100 responses of joint sequences (SDs in parentheses)

	Individual-Slow	Individual-Fast	Joint-Confederate	Joint-Computer
Digram Use	0.264 (0.024)	0.277 (0.036)	0.237 (0.021)	0.236 (0.019)
Adjacency	13.76 (6.92)	21.24 (7.89)	17.62 (2.49)	19.05 (3.35)
Immediate Repetition	0.737 (1.445)	0.868 (1.880)	7.36 (2.06)	6.30 (2.15)
Redundancy	0.604 (0.476)	0.925 (0.857)	0.937 (0.398)	0.902 (0.469)

Randomness scores in joint conditions are the mean scores of those calculated from the first and latter 100 responses of joint sequences.

Table 3. Means randomness scores based on 100 responses of only participants in joint sequences (SDs in parentheses)

	Participant in Joint-Confederate	Participant in Joint-Computer
Digram Use	0.279 (0.025)	0.269 (0.034)
Adjacency	21.71 (6.59)	22.87 (6.84)
Immediate Repetition	0.921 (1.305)	1.000 (1.395)
Redundancy	1.645 (1.446)	1.746 (1.614)

Redundancy scores are the same as those in the bottom right of Table 1.

Figure Captions

Fig. 1 The experimental setup of (a) joint-confederate condition and (b) joint-computer condition

Fig. 2 (a) Repetition lags of joint sequences; (b) Self-repetition lags in joint sequences; (c) Repetition lags of individual sequences

Fig. 3 Repetition lags of participant-in-pair sequences in joint conditions

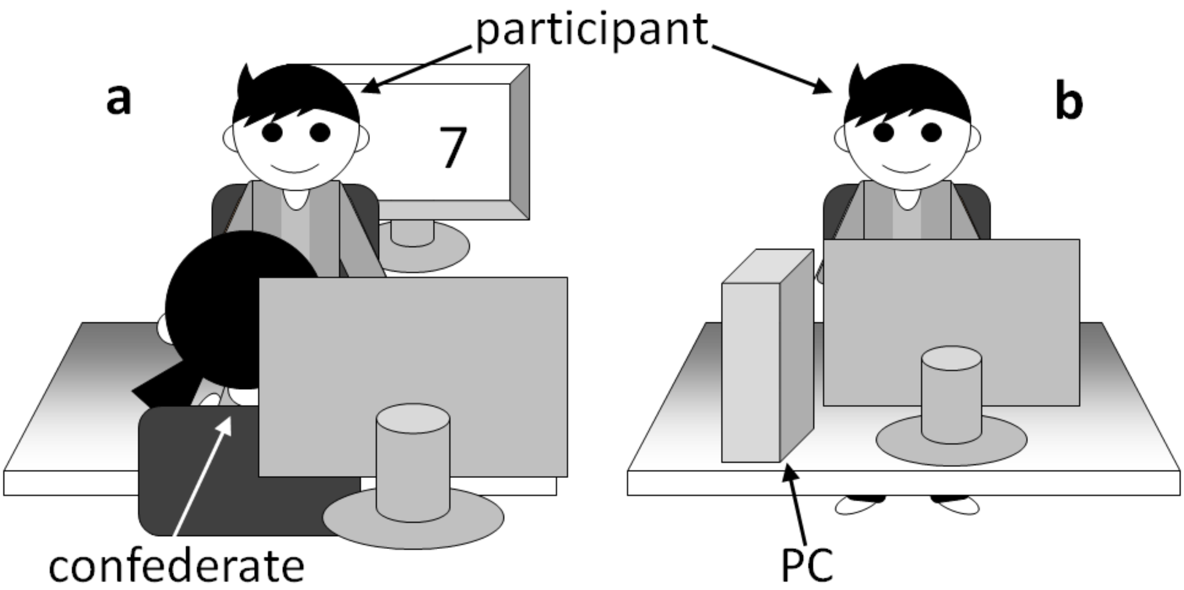

Figure 1

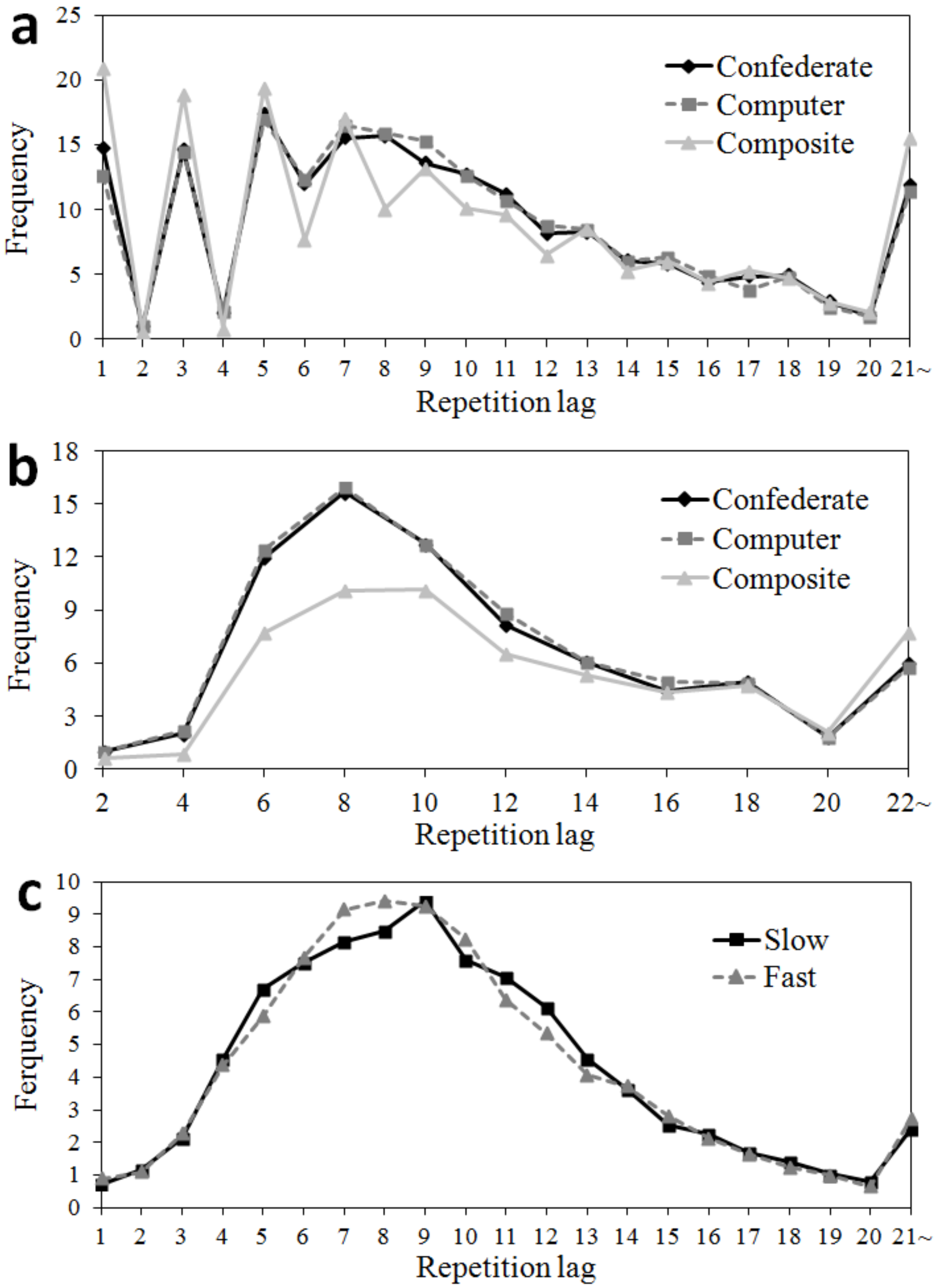
a
Frequency
Confederate
Computer
Composite
Repetition lag
b
Frequency
Confederate
Computer
Composite
Repetition lag
c
Ferquency
Slow
Fast
Repetition lag

Figure 2

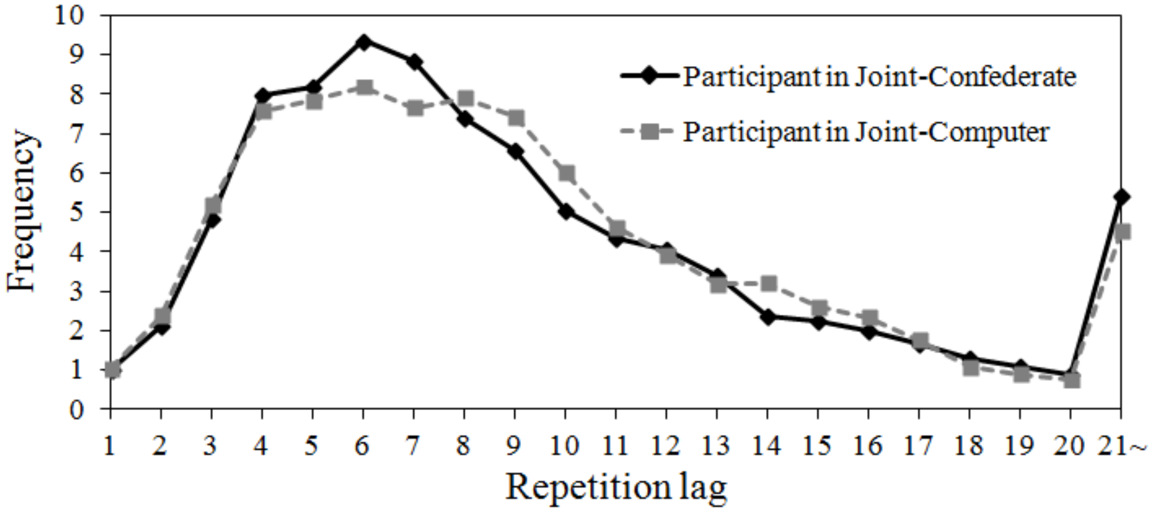
Frequency
0
1
2
3
4
5
6
7
8
9
10
Participant in Joint-Confederate
Participant in Joint-Computer
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21~
Repetition lag

Figure 3

Appendix

Table A1. Statistics of *t*-tests or Wilcoxon's signed rank tests for each performance measure in comparisons between individual and joint sequences (for data in Table 2)

	t-test		
Comparison	t (37)	p	η^2
Digram Use			
Individual-Slow vs. Individual-Fast	1.90	.065	.090
Individual-Slow vs. Joint-Confederate	5.18	< .001	.423
Individual-Slow vs. Joint-Computer	5.64	< .001	.462
Individual-Fast vs. Joint-Confederate	5.95	< .001	.490
Individual-Fast vs. Joint-Computer	6.48	< .001	.533
Joint-Confederate vs. Joint-Computer	0.08	.940	< .001
Adjacency			
Individual-Slow vs. Individual-Fast	9.63	< .001	.723
Individual-Slow vs. Joint-Confederate	3.92	< .001	.292
Individual-Slow vs. Joint-Computer	5.16	< .001	.423
Individual-Fast vs. Joint-Confederate	3.30	.002	.230
Individual-Fast vs. Joint-Computer	1.86	.070	.084
Joint-Confederate vs. Joint-Computer	3.20	.003	.221
Immediate Repetition			
Individual-Slow vs. Individual-Fast	0.66	.515	.012
Individual-Slow vs. Joint-Confederate	18.72	< .001	.903
Individual-Slow vs. Joint-Computer	15.64	< .001	.865
Individual-Fast vs. Joint-Confederate	15.14	< .001	.865
Individual-Fast vs. Joint-Computer	14.23	< .001	.846

Joint-Confederate vs. Joint-Computer	2.36	.024	.130
	Redundancy		
Individual-Slow vs. Individual-Fast	2.09	.044	.109
Individual-Slow vs. Joint-Confederate	3.34	.002	.230
Individual-Slow vs. Joint-Computer	3.14	.003	.212
Individual-Fast vs. Joint-Confederate	0.09	.931	< .001
Individual-Fast vs. Joint-Computer	0.17	.867	< .001
Joint-Confederate vs. Joint-Computer	0.45	.656	.005

Wilcoxon's signed rank test

Comparison	*T*	*z*	*p*
	Immediate Repetition		
Individual-Slow vs. Individual-Fast	46.00	0.82	.414
Individual-Slow vs. Joint-Confederate	0.00	5.38	< .001
Individual-Slow vs. Joint-Computer	0.00	5.38	< .001
Individual-Fast vs. Joint-Confederate	0.00	5.38	< .001
Individual-Fast vs. Joint-Computer	0.00	5.31	< .001
Joint-Confederate vs. Joint-Computer	170.00	2.19	.029

Table A2. Statistics of t-tests for the frequency of repetition lag at each lag value in comparisons between confederate, computer, and composite sequences in Fig. 2a

Comparison	t (37)	p	η^2
Lag 3			
Joint-Confederate vs. Joint-Computer	0.22	.831	.002
Joint-Confederate vs. Composite	3.83	< .001	.281
Joint-Computer vs. Composite	4.10	< .001	.314
Lag 4			
Joint-Confederate vs. Joint-Computer	0.53	5.99	.008
Joint-Confederate vs. Composite	3.61	.001	.260
Joint-Computer vs. Composite	4.15	< .001	.314
Lag 5			
Joint-Confederate vs. Joint-Computer	0.52	.606	.008
Joint-Confederate vs. Composite	1.91	.064	.090
Joint-Computer vs. Composite	2.96	.005	.194
Lag 6			
Joint-Confederate vs. Joint-Computer	0.86	.398	.020
Joint-Confederate vs. Composite	9.43	< .001	.706
Joint-Computer vs. Composite	7.75	< .001	.624